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A NATURALIST'S YEAR.

By GRANT ALLEN.

XVI.—ABOUT VARIATION.

STROLLING along the lane by the mill-stream this afternoon, I see with a shudder that the epilobes and the St. John's worts are coming out in blossom again for the season, and I feel dimly conscious in my own heart that the annual task of trying to sort them out decently into well-marked divisions will recur once more in full tediousness. They are a hopeless set of interlacing species, these weedy, wayside summer flowers; a perfect chaos of gradually-merging characteristics, each supposed kind shading off imperceptibly into the next through infinitesimal and imperceptible gradations. Of course, I am personally quite convinced that the effort to introduce an artificial separation where Nature still keeps up an unbroken series is really as absurd as it is futile: but the superstitions of the systematists die hard, and I feel in duty bound to settle in my own mind to which particular bundle of his arbitrary groups a conscientious technical botanist would consider himself at liberty to refer this, that, or the other particular epilobe, or hypericum, or hawkweed. The thing doesn't really matter in the least, being only a question of human naming, not a question of genuine natural distinctness; but if one leaves it undone, one has an unpleasant sense of a social duty unperformed, and an important dogma in the Athanasian creed of conservative science imperfectly understood.

It is in great part the general want of knowledge as to the close way in which many large groups of species thus interosculate that makes most people hesitate so much about accepting the simple truths of evolutionism. The animals with which they are best acquainted—and most people look at the matter from the standpoint of animal life alone—are a few big mammals, all readily distinguishable from one another, at least here and now, and with few remaining intermediate links to bridge over the gaps between them. Nobody is in any danger of mistaking a cow for a sheep, or even a horse for a donkey; and the various tarpans, and onagers, and quaggas, and zebras, which span the gulf in the last instance and do really merge into one another very indefinitely, are only to be seen in the Zoological Gardens, or else in Tartary, Central Africa, and other reasonably inaccessible parts. But when we come to examine all the known species of any great group, especially among plants, the real difficulty is often not to find intermediate links, but to discover any well demarcated lines of division, or any constant assemblage of characters marking the artificial bundles into which we choose to distribute them. Our English flora is a small and fragmentary one, so that this difficulty is not felt within its narrow limits so greatly as when we examine wider areas; but whenever one comes to compare together a large number of specimens from all parts of the world, it is wonderful how extremely elusive is the task of species-making. True species do often undoubtedly exist: that is to say, there are certainly many groups in which the various existing individuals fall naturally into well-marked divisions, all the intervening types having been killed out by the competition of the better-adapted specific forms, as Mr. Darwin has pointed out. Still, many other groups, perhaps almost as numerous (among plants, at least), do also exist, in which there is no such distinct demarcation of species: the various typical forms merge on either side into one another by every possible intermediate variation. Perhaps these are instances of species in the making; perhaps they show us that primitive plastic state during which variations have not yet become fixed and definite; but, in any case, there they are, and even the most rigorously orthodox botanists can hardly deny their existence.

The local British interest of the epilobes, the St. John's worts and the hawkweeds, consists in the fact that they display just this close interosculation of species even within the range of our own narrow English flora. On the one hand, nobody could fail to recognise the difference between some of the most distinct types—for example, the great purple willow herb, or the pretty pink rose-bay, as compared with the hoary epilobe and the square epilobe of our wayside ditches; or again, the great St. John's wort of our gardens, bearing flowers three or four inches in diameter, as compared with the scrubby, small-flowered *Hypericum dubium* and *Hypericum quadrangulum*, that line our dusty country roads. But, on the other hand, if one attempts to divide them all up into distinct species, one finds oneself hampered by all kinds of intermediates, which make the definite classification of the text-books practically impossible in actual practice. Indeed, the text-books themselves all differ as to the number of species they admit; and though some botanists will at once decide offhand by what name they would call a particular specimen, there are other wiser and more cautious authorities who will not thus commit themselves to a fixed and dogmatic nomenclature. I turn to Dr. Bentham on these very epilobes, and I find he truly remarks: "The numerous forms the species assume in every variety of climate make it exceedingly difficult to define them upon any certain principle, and botanists seldom agree as to the number they should admit. Those here adopted are the most marked among our British forms; but it must be confessed that in some instances intermediates are to be met with which will be found very puzzling." He then goes on to say that in all cases the style should be carefully observed when fresh, in order to see whether it is entire or four-lobed; but even this distinction, upon which he bases his table of species, is a very unimportant one; for in most epilobes the four lobes of the style are joined in a club-shaped head at an early stage in the bud, and only open as the flower matures; while even in those species where they never open, the marks of the four cohering lobes may be easily seen with

XUM

a small pocket lens. The opening is, in fact, merely one of the ordinary devices to prevent self-fertilisation; it is found in all the larger and handsomer British species, which specially lay themselves out to attract insect visitors, while it is wanting in the smaller and less conspicuous varieties. Nor is this all, for in the little intermediate type, known as *Epilobium roseum*, the lobes sometimes open shortly, and sometimes remain closed in a club-shaped stigma; and I have found flowers in both states growing together on the same plant.

A rapid glance at the various forms assumed by British representatives of this Protean genus will help better than any amount of generalised talk to explain exactly how closely the different groups resemble one another. Omitting the naturalised French willow or rose-bay (which I do not believe to be truly indigenous anywhere in Britain), we have four fairly distinct central types of native epilobes, as it seems to me, usually distributed into from seven to eleven or more recognised species. Of these the largest, handsomest, and, on the whole, most marked type, is the great purple-red willow-herb (*Epilobium hirsutum*), with a bunch of very big and showy flowers. This is the great insect-fertilised form, laying itself out most for attractive display, and having the stigma deeply four-lobed. Below it comes the so-called broad epilobe (*E. montanum*), differing from it, in good typical specimens, by its stalked and rounded leaves, while those of the willow-herb are longer, narrower, and more sessile on the stem, as well as by its much smaller and pinker flowers, still, however, with a four-lobed stigma. This is a less luxuriant form of the same general type, growing in dry situations, while the willow-herb affects moist ditches; and it shows some signs of incipient degeneration in its short petals, sometimes twice as long as the calyx, but often only of about the same length. These two forms are universally admitted as distinct. But intermediate between them come the specimens known as *E. parviflorum*, some of which closely resemble the willow-herb in leaves and flowers, while others approach very near to *E. montanum*. So far as my observation goes, in very wet situations, they verge in the direction of the larger type, and in very dry ones of the smaller. I may add that dwarfed specimens of *E. montanum*, growing on the top of walls or other waterless spots, carry the characteristics of the small type to an extreme. But between these intermediates, even, it is possible to find yet other intermediates, approximating still more closely to the upper or lower forms. So much for the two main large-flowered types with four-lobed stigmas; next as to the two main small-flowered types, with a club-shaped style. One of these is a lowland form (*E. tetragonum*); the other is a low, tufty mountain plant (*E. alpinum*). But *E. tetragonum*, once more, which in very typical specimens is known by its square stem, marked with four decurrent lines, is linked to the four-lobed species by the intermediates known as *E. roseum*, some of which have the style club-shaped, while others have it slightly opened into four short lobes. This *E. roseum* itself merges into *E. montanum*, on the one hand, by the small-flowered dry-soil specimens mentioned above, through the shortly four-lobed form called *E. lanceolatum*, and into the regular *E. tetragonum*, on the other hand, by specimens with entire stigmas and marked decurrent lines. Other small differences, which I need not note here, have made some divergent plants of *E. tetragonum* be considered as specifically distinct, under the name of *E. virgatum*. Finally, the dwarf mountain type, *E. alpinum*, when growing under exceptionally favourable circumstances, by the banks of Highland streams, assumes longer and broader leaves, toothed on the edges, and recurs somewhat towards the lowland type, in which state it is known as *E. alsinæfolium*; while in boggy places on lower ground, it reverts towards the square-stemmed form, and receives the name of *E. palustre*. "It sometimes, also, comes very near the narrow-leaved forms of *E. roseum* and *E. tetragonum*," says Mr. Bentham, "but has the buds much more nodding, and the decurrent lines on the stem are either very faint or entirely wanting." For my own part, I have seen intermediate specimens between almost all these carefully-distinguished species or varieties which it would be mere juggling, in my opinion, dogmatically to refer to either or any type. But if ever the various intermediate forms are weeded out by the action of natural selection, there will probably remain four "good species," as systematists call them:—*E. hirsutum*, *E. montanum*, *E. tetragonum*, and *E. alpinum*.

This is rather a long, dull, and technical exposition, I am aware; but it will serve better than anything else to show how impossible is the attempt to draw definite lines in such very mixed and elastic families. Nor are these families by any means exceptional. To anybody who wishes for an insight into the variety and modifiability of plant forms even within the limits of a single small country, the careful comparative study of the following common English genera and species may be confidently recommended: the reses (*Rosa canina*, *R. arvensis*, &c. Mr. Bentham makes five species, and Mr. Babington, seventeen); the brambles (*Rubus*, from four to forty-five species recognised); the potentillas (including *Tormentilla* and the strawberry); the St. John's worts (*Hypericum*); the sow-thistles (*Sonchus*) and the hawkweeds (*Hieracium*). How any one can rise from a close comparison of the endless forms of hawkweed alone, a believer in strictly defined natural species, is one of those psychic mysteries only adequately to be explained by Mr. Bishop, or by Messrs. Maskelyne & Cooke.

THE CHEMISTRY OF COOKERY.

XIII.

By W. MATTIEU WILLIAMS.

THE process of frying follows next in natural order to those of roasting and grilling. A little reflection will show that in frying the heat is not communicated to the food by radiation from a heated surface at some distance, but by direct contact with the heating medium, which is the hot fat commonly, but erroneously, described as "boiling fat."

As these papers are intended for intelligent readers who desire to understand the philosophy of the common processes of cookery, so far as they are understandable, this fallacy concerning boiling fat should be pushed aside at once.

Generally speaking, ordinary animal fats are not boilable under the pressure of our atmosphere (one of the constituent fatty acids of butter, butyric acid, is an exception; it boils at 314° Fah.). Before their boiling-point, *i.e.*, the temperature at which they pass completely into the state of vapour, is reached, their constituents are more or less dissociated or separated by the repulsive agency of the heat, new compounds being in many cases formed by recombinations of their elements.

When water is heated to 212° it is converted completely into a gas which gas returns to the fluid state without any loss on cooling below 212°. In like manner if we raise an essential oil, such as turpentine, to 320°, or oil of peppermint to 340°, or orange peel oil to 345°, or patchouli to 489°, and other such oils to various other temperatures,

they pass into a state of vapour, and these vapours when cooled, recondense into their original form of liquid oil without alteration. Hence they are called "volatile oils," while the greasy oils which cannot thus be distilled (in which animal fats are included), are called "fixed oils."

A very simple practical means of distinguishing these is the following:—Make a spot of the oil to be tested on clean blotting-paper. Heat this by holding it above a spirit lamp flame, or by toasting before a fire. If the oil is volatile, the spot disappears; if fixed, it remains as a spot of grease until the heat is raised high enough to char the paper, of which charring (a result of the dissociation above-named) the oil partakes.

But the practical cook may say "this is wrong, for the fat in my frying-pan does boil, I see it boil, and I hear it boil." The reply to this is, that the lard, or dripping, or butter that you put into your frying-pan is oil mixed with water, and that it is not the oil but the water that you see boiling. To prove this, take some fresh lard, as usually supplied, and heat it in any convenient vessel, raising the temperature gradually. Presently, it will begin to splutter. If you try it with a thermometer you will find that this spluttering point agrees with the boiling point of water, and if you use a retort you may condense and collect the splutter-matter, and prove it to be water. So long as the spluttering continues the temperature of the melted fat, *i.e.*, the oil, remains about the same, the water vapour carrying away the heat. When all the water is driven off the liquid becomes quiescent, in spite of its temperature, rising from 212° to near 400°, then a smoky vapour comes off and the oil becomes darker; this vapour is not vapour of lard, but vapour of separated and recombined constituents of the lard, which is now suffering dissociation, the volatile products passing off while the non-volatile carbon (*i.e.*, lard-charcoal) remains behind, colouring the liquid. If the heating be continued, a residuum of this carbon, in the form of soft coke or charcoal, will be all that remains in the heated vessel.

We may now understand what happens when something humid—say a sole—is put into a frying-pan which contains fat heated above 212°. Water, when suddenly heated above its boiling point is a powerful explosive, and may be very dangerous, simply because it expands to 1,728 times its original bulk when converted into steam. Steam-engine boilers and the boilers of kitchen stoves sometimes explode simply by becoming red-hot while dry, and then receiving a little water which suddenly expands to steam.

The noise and spluttering that is started immediately the sole is immersed in the hot fat is due to the explosions of a multitude of small bubbles formed by the confinement of the suddenly expanding steam in the viscous fat from which it releases itself with a certain degree of violence. It is evident that to effect this amount of eruptive violence, the temperature must be considerably above the boiling-point of the exploding water. If it were only just at the boiling-point, the water would boil quietly.

As we all know, the flavour and appearance of a boiled sole or mackerel are decidedly different from those of a fried sole or mackerel, and it is easy to understand that the different results of these cooking processes are to some extent due to the difference of temperature to which the fish is subjected.

The surface of the fried fish, like that of the roasted or grilled meat, is "browned." What is the nature, the chemistry of this browning?

I have endeavoured to find some answer to this question, that I might quote with authority, but no technological or purely chemical work within my reach supplies such answer. Rumford refers to it as essential to roasting, and provides for it in the manner already described, but he goes no further into the philosophy of it than admitting its flavouring effect.

I must therefore struggle with the problem in my own way as I best can. Has the gentle reader ever attempted the manufacture of "hard-bake," or "toffy," or "butter-scotch," by mixing sugar with butter, fusing the mixture, and heating further until the well-known hard, brown confection is produced. I venture to call this fried sugar. If heated simply without the butter it may be called baked sugar. The scientific name for this baked sugar is *caramel*.

The chemical changes that take place in the browning of sugar have been more systematically studied than those which occur in the constituents of flesh when browned in the course of ordinary cookery. Believing them to be nearly analogous, I will state, as briefly as possible, the leading facts concerning the sugar.

Ordinary sugar is crystalline, *i.e.*, when it passes from the liquid to the solid state it assumes regular geometrical forms. If the solidification takes place undisturbed and slowly, the geometric crystals are large, as in sugar-candy; if the water is rapidly evaporated with agitation, the crystals are small, and the whole mass is a granular aggregation of crystals, such as we see in loaf sugar. If this crystalline sugar be heated to about 320° F. it fuses, and without any change of chemical composition undergoes some sort of internal physical alteration that makes it cohere in a different fashion. (The learned name for this is *allotropism*, and the substance is said to be *allotropic*, other conditioned; or *dimorphic*, two-shaped.) Instead of being crystalline the sugar now becomes vitreous, it solidifies as a transparent amber-coloured glass-like substance, the well-known barley sugar, which differs from crystalline sugar not only in this respect, but has a much lower melting-point; it liquefies between 190° and 212°, while loaf-sugar does not fuse below 320°. Left to itself, vitreous sugar returns gradually to its original condition, loses transparency, and breaks up into small crystals. In doing this, it gives out the heat which during its vitreous condition had been doing the work of breaking up its crystalline structure, and therefore was not manifested as temperature.

This return to the crystalline condition is retarded by adding vinegar or mucilaginous matter to the heated sugar, hence the confectioners' name of "barley sugar," which, in one of its old-fashioned forms, was prepared by boiling down ordinary sugar in a decoction of pearl barley.

The French cooks and confectioners carry on the heating of sugar through various stages bearing different technical names, one of the most remarkable of which is a splendid crimson variety, largely used in fancy sweetmeats, and containing no foreign colouring matter, as commonly supposed. Though nothing is added, something is taken away, and this is some of the chemically-combined water of the original sugar, in the parting with which not only a change of colour occurs, but also a modification of flavour, as anybody may prove by experiment.

When the temperature is gradually raised to 420°, the sugar loses two equivalents of water, and becomes *caramel*—a dark-brown substance, no longer sweet, but having a new flavour of its own. It further differs from sugar by being incapable of fermentation. Its analogies to the crust of bread and the "brown" of cooked animal food will be further discussed in my next.

ACCORDING to Mr. Kolb, one of the secretaries of the German Imperial Telegraph Department, the 127,166 galvanic cells in use cost £12,350, of which £2,727, or about 22 per cent., were recovered by the sale of the battery residues, consisting of copper, zinc, and lead salts.

XUM

GEOLOGY AND AGRICULTURE.

BY J. VINCENT ELSDEN, B.Sc. (Lond.), F.C.S.

IN a country like England, in which a day's journey is sufficient to traverse rocks of every geological age, even the most careless observer cannot fail to be struck by the diversity of appearance presented by the different strata over which he travels. The stiff soils of the eastern boulder-clays, the sheep-farming of the chalk downs, the pasture-land on the cold clays of the Midland Counties, the rich land on the Old Red Sandstone, and the barren aspect of the rugged Silurian rocks of Wales all point to the fact that each of the great geological systems is characterised by peculiar agricultural features. But, in England, much of the contrast between the various formations is lost owing to the great extent to which the underlying rocks are covered up by superficial accumulations, which entirely change the character of the soil.

In Belgium similar differences are to be observed between the desolate plateau formed by the Palæozoic rocks of the Condroz and Ardennes, the fertile Hesbayan loam of the central parts, the barren Campine sands, producing only heather and pines, and the rich alluvial soil of the Polders.

In Norway, the fjelds, or elevated plateaux, which occupy more than half its area, are perfectly barren; and, in Sweden, the cultivated tracts coincide with the deposits of glacial clay and marl, which cover up the gneiss and granite.

From the alluvial deposits forming the two immense plains of Hungary, one of the richest soils of Europe has been formed, and the chief source of the agricultural wealth of Russia is the recent deposit, known as the Tchornoi-zem, or black earth, which occupies the valleys of the Don, Dnieper, and Volga.

In America, the connection between geology and agriculture is still more marked. The sudden transition from the fertile alluvium of Virginia to the barren sands, clothed with pine forests, which characterise the Tertiary beds; the dry chalk downs and treeless prairies, famous for Georgian wheat, which mark the Secondary deposits, and the general husbandry and fertile soils of the Primary rocks, forming the lower parts of the Alleghanies, show the geological features of the country almost as perfectly as would a careful survey of the underlying rocks.

It is not to be supposed, however, that these differences in agricultural features are due to the character of the soil alone, for climate has as much influence as soil in modifying agricultural operations; but since climate is regulated mainly by contour, and contour depends chiefly upon geological structure, there is the same primary cause for variations in both cases. Nor must it be forgotten that it is the tendency of improved systems of agriculture to overcome all obstacles to the growth of crops, even in soils which are naturally unsuited to them; and thus the natural diversity of character which formerly existed is, to a great extent, destroyed. This is especially noticeable in the improvement of large tracts formerly regarded as wastes, and in the breaking-up of permanent pasture. For instance, the agricultural features of the chalk districts of England are by no means so marked, now that they have been invaded by the plough, as they were when in rolling downs and sheep pastures only. But in these cases the improvement is artificial, and the natural conditions would reappear if constant attention were not paid to the maintenance of the soil in an improved state. To the ordinary observer the appearance of a naturally fertile soil, and of a barren soil, rendered artificially productive, may be very much the same; but to the farmer the difference is extreme, for while the former is worked with ease, the latter can only be made to yield good results by endless trouble and expense.

Hence it is seen that not only do the natural agricultural capabilities of the soil vary with the geological structure of the rocks from which it is derived; but also that facilities for improvement and the amount of attention necessary to prevent deterioration are similarly influenced.

The natural vegetation of any locality is so entirely dependent upon the nature of the soil, that the geologist often receives great assistance, in mapping the boundary-lines between different strata, from careful observations of the plants which grow there. For instance, it is most interesting to note how that beautiful heath, *Erica Vagans*, which grows upon the serpentine of the Lizard district, in Cornwall, marks out the boundary of the barren serpentine from the fertile soils of the adjoining rocks. Buckland, writing in 1840, describes a moor in Dumfries, in which a band of bright green herbage marked the course of a trap dyke traversing slate rocks. Similarly, in Staffordshire the line of junction of carboniferous limestone and millstone grit is very clearly drawn on the surface by the sudden change in the quality of the grass, and four plants in particular mark the exact spot where soil changes—the furze, the heath, the whortleberry and the sorrel. In Hertfordshire, the boundary-line of the London clay is frequently defined by a verge of grass-land, which terminates with the out-crop of the chalk; while in the New Forest, the presence of the taller furze is a certain indication to the farmer that the land below is worth reclaiming. In many cases, also, particular formations are characterised by the trees which grow upon them. In Surrey, the Gault is distinguished by the luxuriant growth of oak and elm; while the Weald clay has long been noted for the perfection of its oaks. Beech trees abound on the chalk formation, and elms flourish on London clay, while the famous oaks of Bagot's Park, in Staffordshire, mark the position of an outlying patch of Lias. It is well-known that the districts most celebrated for their cider are situated on the cornstones and marls of the Old and New Red Sandstone formations, and, in Scotland, the Carse of Gowrie, so famous for its apples, stretches over the Old Red strata.

To such an extent, indeed, is vegetation influenced by soil and climate, that the experienced farmer can form an opinion as to the fertility of the land from the species of plant growing upon it. For this reason many parts of the British Isles have remained to this day untilled, forming the moorlands and heath-covered wastes so characteristic of many geological strata. But even a naturally fertile soil is frequently rendered unproductive by deficient water-supply or defective drainage. The scarcity of water in chalk districts is a source of continual trouble and expense to the farmer, who is obliged to sink deep wells, at a very great cost, or to depend upon the supply afforded by artificially constructed dew-ponds. In Alabama, the dryness of the chalk soils is a great hindrance to agriculture, for only farmers of large means can afford to undertake the deep boring necessary to obtain a supply of water. A similar scarcity of water is experienced on account of the extreme porosity of the Oolitic soils of the Cotteswolds. On the other hand, a farmer who is located upon impervious clays is put to no less an expense in getting rid of superfluous water by draining his land. Even upon porous rocks like the Old Red Sandstone, bogs are frequent on account of the sand becoming cemented into a hard mass, by means of oxide of iron, thus forming an impermeable pan.

(To be continued.)

LAWS OF BRIGHTNESS.

V.

By RICHARD A. PROCTOR.

THE law obtained at page 372 relates to smooth spheres, or spheres having a *mat* surface (German, matt, *dead*, as applied to surfaces), *unpolished*. It is useful to know the law, as it affords a means of determining how far the observed changes of brightness of those planets which present phases accord with the changes which would result if the surfaces of the planets were smooth.

To begin with the moon :—

Now here, before proceeding to consider the moon's total brightness at her phases, we find ourselves confronted by a circumstance which in no sense accords with what was shown in the preceding part of this paper, as to the brightness of a smooth sphere illuminated directly. We see that the disc of the full moon does *not* present that shading off towards the edge or limb which theory requires in the case of a smooth sphere. Setting aside the remarkable variety of brightness seen in different parts of the moon's disc, we find no perceptible tendency to diminution of brightness towards the moon's limb. If we look at the moon through a tinted glass suitably graduated, we find that the outline remains distinctly visible as long as any part of the moon (except, of course, the exceptionally bright spots) can be seen.

It is obvious that we must look for the explanation of this circumstance in the unevenness of the moon's surface. We can easily see, for instance, that if the moon's surface were covered *all over* with steep conical hills, something like the observed result would follow. For let *a*, *b*, *c*, Fig. 11, be steep conical hills on a globe, supposed to be illuminated from above ; the light rays meet the surface at

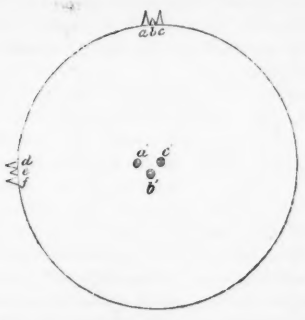

Fig. 11.

a small angle, and the illumination is therefore small ; hence, supposing these hills in the centre of the disc (that is, the globe "full"), they would appear as three dark spots, as *a'*, *b'*, *c'* ; and if the whole surface of the globe were covered with such hills the whole of the middle part of the disc would be dark, comparing its light with that from the middle of a smooth sphere of the same substance and under equal illumination. Such hills on the edge of the disc, as at *d*, *e*, *f* (and supposed to be illuminated in the same direction that the globe is viewed) would not appear uniformly bright, for at their edges they would be under very oblique illumination, while along a central streak the illumination would be nearly square (the hills being steep) ; but it is easily seen that the total illumination of a hill-covered region so placed might be (according to the slope of the hills) rather greater than, or very little less than, the illumination of the spots, *a'*, *b'*, *c'*.

It appears to me, however, that it is on the whole more satisfactory to regard the general unevenness of the moon's surface as due rather to crateriform elevations than to conical hills. (It must be noticed that we are not concerned here with the features revealed by the telescope ; since the nature of the illumination of different parts of the full moon must be mainly due to irregularities not discernible by the telescope.) Now, if we take a crater having such a section as is shown in Fig. 12, we see that under vertical illumination we should have the base C D fully illuminated, and the slopes B A, E F somewhat

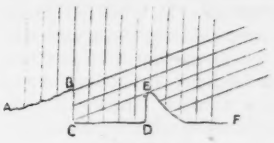

Fig. 12.

obliquely illuminated ; while under the oblique illumination from the right, indicated by the transverse lines, we have B C and E F (the only parts under illumination), both illuminated nearly squarely. Of course, the latter remarks only relate to one cross-section of the crater ; but it is easily seen that the oblique illumination may give the same apparent illumination for the crater (regarded as a whole, and viewed in the same direction that the light falls) that direct illumination gives. If C D be of less reflective power than A B C and D E F, of course the oblique illumination would have a further advantage, the direction of the line of light being such that the dark bottom of the crater would be concealed behind the wall D E F.

I forbear from entering further, however, into such considerations as these, simply because the varieties and combinations of slope, position, tint, &c., are endless. It suffices here that the brightness of the moon's disc near the edge is explicable in a general way by the circumstance that the moon's surface is uneven.

It will be obvious that the total light received from the moon must necessarily be affected by a condition of her surface which renders the seeming illumination of different parts of her disc so different from that which would result were she smooth.* It is not easy to determine to what

* Some very strange assertions are made in a section of the "Encyc. Brit." devoted to the question of the moon's brightness. They illustrate what I have said respecting the errors commonly made on the subject of brightness, and I therefore quote the passage at length. The writer of the passage has given certain arguments (based on Bouguer's and Leslie's inexact estimates of the moon's total brightness) which appear to show that the moon is self-luminous. He proceeds as follows :—"Although these arguments go far to support the ancient opinion of the native light of the moon, they are not entirely conclusive, and indeed cannot be easily reconciled with some of the phenomena. If the moon shines in virtue of her native light, rays will be emitted in all directions from every point of her surface ; whence, since a visual angle of a given magnitude includes a much larger portion of a spherical surface near the extremities of its apparent disc than towards the centre, and as the number of rays is proportional to the surface from which they proceed, it follows that the intensity of the moon's light ought to be greater near the border than at the centre of her disc." (The writer was clearly unacquainted with the experimental law of the emission of light from self-luminous bodies). "The reason why this is not the case with regard to the sun" (he should have said, the reason why a variation of the opposite kind exists) "is that a greater proportion of the rays are absorbed in passing through a greater extent of the solar atmosphere ; but the moon, having no atmosphere, ought to be sensibly most brilliant near the circumference of her orb. The contrary is, however, the case ; her light is greatest at the centre and less intense towards the circumference (!), exactly as it ought to be on

degree the total quantity of light would thus be affected. Zöllner considers that the full moon shines nearly as brightly as though she were a flat disc under full solar illumination.

(To be continued.)

THE AMATEUR ELECTRICIAN.

BATTERIES.—II.

THE Bunsen cell is one which may be regarded as a "typical" cell, and accordingly it is proposed to deal with it in this article. It must not, however, be supposed that all has been said that we intended to say on the Daniell cell. There are certain modifications of it which only require to be more extensively known, in order for them to receive a much larger share of patronage. Our purpose in first dealing with typical cells is to afford, subsequently, ready means of comparison, as well as to save time and space, which would otherwise be occupied with repetitions which our present plan renders unnecessary.

The Bunsen cell consists of an earthenware jar, generally cylindrical, into which is placed a cylinder of zinc, made by rolling a piece of flat zinc into the required cylindrical form, but not allowing the two approaching edges to quite meet. The object sought is to facilitate the movement of the solution in which the metal is immersed. Within the cylinder is a round porous pot containing a rod of gas carbon. Brass terminals or binding screws are fitted on to the zinc and carbon, for purposes of connection. Concentrated nitric acid (specific gravity 1·420) is put into the porous pot or the division of the cell containing the carbon rod or plate. In the outer or zinc division is poured dilute sulphuric acid, or rather acidulated water. When the highest efficiency of the cell is required, the solution contains one of acid to seven of water, but this is a proportion not to be recommended, as it causes an extravagant waste of zinc by local action, of which more will be said below. A very fair proportion is one of acid to twelve or fifteen of water. Hydrochloric (or muriatic) acid (HCl) may be substituted for the sulphuric, but it is a little more expensive.

The action in the cell approaches, to some extent, the action in a Daniell cell. The zinc is dissolved and converted into sulphate of zinc (SO_4Zn), the hydrogen of the sulphuric acid (SO_4H_2) being set free. This, entering the porous pot, decomposes the nitric acid (NO_3H), forming water (OH_2) and nitric peroxide (N_2O_4). Expressed chemically, we get

$$Zn+SO_4H_2+2NO_3H=SO_4Zn+2OH_2+N_2O_4$$

Here it will be seen that when one volume of zinc is dissolved, two volumes of nitric acid are decomposed, and that for each double volume of nitric acid decomposed, a double volume of water is formed which dilutes the remaining acid, a volume of nitric peroxide being also produced. Thus the quantity of the nitric acid is being constantly reduced by the decomposing action, while that which remains is subjected to the weakening effect of water. At the same time the acid is further impaired by impregnation with nitric peroxide, which, also getting into the pores of the carbon, doubtless tends to produce a polarising effect by coating the particles of carbon. A large quantity of the nitric peroxide escapes into the air, and has a highly deleterious effect upon the human or any other animal system. It is apparent, therefore, that the Bunsen cell differs from the Daniell cell in one of the most important features. In the cell last mentioned, the strength of the cupric sulphate (SO_4Cu) solution remains constant, being maintained by a reservoir of bluestone crystals, but a similar reservoir of nitric acid is impossible. Even were it otherwise, the production of water produces a weakening effect, which can only be obviated by withdrawing both acid and water. The Bunsen cell is, therefore, inconstant, the strength of the current diminishing with the time. Presuming the porous pot to be well saturated, the diminution of the current theoretically commences the moment the circuit is completed, and in a few minutes it may make itself perceptible. A Bunsen cell, consequently, cannot be relied upon for more than three or at the outside four hours. At the end of that time, the nitric acid requires to be replaced by fresh acid; but the old acid may be used again twice or perhaps three times if it is allowed to stand aside for awhile to clarify it.

In the outer division the zinc is rapidly dissolved, and requires to be occasionally carefully amalgamated, otherwise, however good the zinc may be, local action—that is to say, action between two points on the zinc—will set in. Wherever the metal has two different structures or molecular arrangements, or has a chemically impure surface, there will local action be seen. One has only to place an unamalgamated zinc in a cell for an hour or so to see this. On taking the metal out it will be found to be eaten away very unevenly—sometimes in holes, sometimes in longitudinal striations.

Amalgamation is a process which homogenises the structure of the metal, and covers up all superficial impurities. The process may be carried out in a variety of ways. The metal surface is first cleaned by immersion in acidulated water. When the violent fizzing that accompanies the action of the water upon a bar or dirty piece of zinc has nearly subsided, the plate is coated with mercury. This is sometimes accomplished by placing the zinc in a solution of a mercury salt; but the readiest method is to coat the metal with the mercury either by rubbing it in with a piece of tow or cotton-wool, or by rolling the zinc in it. In the latter case the zinc should be allowed to stand a short time in a dish, &c., to drain off superfluous mercury. A pound of mercury is sufficient to roll a six-inch cylinder in, and will last a very long time. Zinc so treated infallibly works more economically than unamalgamated zinc, providing the entire surface of the metal is so treated. The reason assigned is, that the mercury associates itself with the zinc and presents to the solution a surface which is absolutely homogeneous, and which, therefore, does not favour local action. When the metal becomes dull, reamalgamation is necessary.

The cost of making a Bunsen cell is small. One of about a quart capacity is the most generally useful. The outer jar would cost a few pence, as would also the porous pot. The carbon rob, which may be about an inch square and seven inches, or thereabouts, in length, can generally be obtained at the rate of a penny an inch. The cost of the carbon is almost exclusively due to the hard nature of

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the supposition of its being occasioned by the reflection of the solar light." I do not know the author of the passage. I may take this opportunity of noticing that in a passage of an article on the "Indications by Phenomena of Atmospheres to the Sun, Moon, and Planets," in the *Monthly Notices* of the Astronomical Society for 1862-3, page 237, Prof. Challis, while correctly stating the law for the case of a self-luminous body, falls into an error (which the above writer avoids) when speaking of opaque illuminated bodies. He says "a distant spherical body, whether self-luminous or shining by reflected light, would, according to the law that the intensity of the radiation varies as the sine of the angle which the direction of emanation makes with the surface, appear equally bright at all points of the disc." Those correspondents, therefore, who have addressed letters to me asking whether an opaque body under full illumination should not so appear, will see that their mistake is one easily fallen into.

the substance, which can, generally speaking, be obtained gratis from a gas company's works. It is one of the waste products in gas manufacture, and has no other marketable value. Anent the zinc, our remarks in the preceding article are applicable, but a little word may be added here. If the amateur has a small pocket, and is unable to get ¼-inch zinc bent, he may use ⅛-inch, and by raising it to a fairly high temperature, he will find it capable of being easily bent. The retail price for a cylinder weighing a little over a pound is 1s. 9d., or about three times the price of the metal.

Amateurs often complain that the zincs get dirty, and covered with a white salt when put away after using them. They will experience no such trouble if they will remove the zinc from the solution when the battery is done with, and, after carefully and thoroughly rinsing it with water, wipe it dry with a piece of common calico, and then place it in a cupboard or other receptacle apart from the porous pot (which may be advantageously left in the acidulated water) and the carbon block.

The resistance of a Bunsen cell is practically nil, being less than the tenth of an ohm. The electro-motive force is, on the other hand, high, being 1·9 or 2·0 volts.

The cell is useful for laboratory or other experimental work, on account of the intense current yielded by it; but, on account of its rapid polarisation, it is useless where constancy is required.

The Grove cell is similar to the Bunsen, the difference being that the former contains a platinum foil in the place of carbon. Its efficiency is about the same as the Bunsen, but it is cleaner, and, being generally made in the flat form, takes up less room. Its cost is about 50 per cent. more than that of the Bunsen.

SAVAGE AND ANTHROPOLOGIST.

WHEN the "Special Extra Meeting of the Anthropological Institute" was held at the Piccadilly Hall last week, the Botocudos must have been intensely amused. It must not be imagined that the interviewing in this matter is all on the side of the European. Mr. H. M. Stanley has recorded how, in his South African wanderings, he was persistently humiliated by the consciousness that while he thought he was discovering the savages, the savages would insist upon discovering him. When he visited one of their encampments, they surrounded him as an unusually tame specimen of a peculiar pale-coloured variety of human being. They took off his clothes to look at his skin, and noted with satisfaction that he would eat the ordinary viands of life. Thus it frequently happened that Stanley departed from one of their "towns" with the sorrowful knowledge that the anthropologically-minded Africans had picked up more information about him than he had about them. The same fate befell the Man among the Monkeys. They regarded him as a large and hitherto unknown species of monkey, which "resembled an onion" in so far that it had several skins which peeled off easily. In both these cases, however, the European was one among many, whereas at the Piccadilly Hall the odds are all the other way. Nevertheless, the four Botocudos there evidently consider themselves numerous enough to form a scientific quorum for the discussion between themselves of any more than usually peculiar anthropological curiosity in a tall hat that may chance to enter the hall, with the idea that he is the sight-seer and the Botocudo the sight—and not a very inspiriting one at that. But when the special extra meeting of the Anthropological Institute appeared on the scene, the Botocudos must have been really thankful for the treat provided them. When some Fijian savages, under the guidance of a missionary, for the first time visited an English man-of-war, they could hardly express their gratitude sufficiently. "We beheld the men in great numbers, and very industrious at their work," they said, and "we respected them greatly. We beheld the chiefs of the ship, and reverenced them greatly. We young men of this generation were born in blessed times to see such a ship as this. Our fathers saw no such sight." So doubtless the anthropologists and the Botocudos last week equally congratulated themselves upon their good luck. Never before had the anthropologists seen four Botocudos together, one of them with a large piece of wood stuck in her lip; and never before had four Botocudos seen so uniformly venerable an assortment of white men, the majority with oval pieces of glass fixed by wires in front of their eyes, and some of them quite bald. Their fathers saw no such sight.

Nor is it quite certain that in a cursory comparison the Botocudos would not be able to credit themselves with the superiority. It is true that the old lady of their party has distended her lower lip to the size of a tobacco-pouch, but she has not pinched in her waist to a 6½-inch diameter, nor does her husband wear a shiny black funnel, with a brim to it, upon his head. The daughter, too, has obviously no need of stays; and the son's splay feet have been allowed to tread in Nature's free and easy lines; so that between the swarthy youth inside Piccadilly Hall and the gilded youths upon Piccadilly pavement outside, if an active cockroach were the umpire, there can be little doubt which would be pronounced the most effective "masher." The manners and customs of the white men, too, are worth noticing. When two of them meet a third, one of the two steps forward, says a few words to each of the others, and then they take off their tall hats and wave them gently towards each other's feet; and then they put them on again. After that they catch hold of each other's hands and shake them strongly, grinning all the time as if they were happy. Among the transactions of the Anthropological Institute of Botocudoland it will, no doubt, be recorded that the English are in many ways—according to the report of the Botocudo deputation that explored London in 1883—a peculiar race of men, and that the variety known as "anthropologists" are especially remarkable for their age, their fondness for palavers, and their inquisitiveness.—*Globe.*

SOME kinds of white glass become, in the process of time, more or less deeply coloured under the influence of luminous rays. The most common tints are violet and green. The materials of ordinary glass are somewhat ferruginous and capable of tinging glass with a deep green shade by the protoxide of iron. In order to remove the colouring, peroxide of manganese is added, which changes the protoxide into a sesqui-oxide, which gives a feeble reddish-yellow tint. It is almost impossible to observe the proper proportions of manganese and iron. If there is too much oxide of manganese the glass has at first a violet shade; if there is too much protoxide of iron the glass will be greenish; if all the manganese is reduced to a state of protoxide the glass is colourless. The influence of light and air may gradually bring about a partial oxidation of the protoxide of manganese and a violet colouring which increases with time. The *Chronique Industrielle* says a shade which is due to an excess of manganese is observed in the Pinacothek, at Munich, where the upper windows of the picture-gallery give a very marked violet light which produces a bad effect.

THE FISHERIES EXHIBITION.

NATURAL HISTORY DEPARTMENTS.

BY JOHN ERNEST ADY.

II. (*continued*).

WE now pass on to consider the relation of the minute organisms considered in our last article, to the important formations which have and are still taking place in the bed of the deep sea, and on which so much light has recently been thrown by the late Sir Wyville Thomson, and his associates of the *Challenger* expedition. If a piece of chalk, such as that of Gravesend, be gently powdered in a mortar, the mass tied up in a small bag of coarse calico, and kneaded thoroughly in a large bowl of fresh water until it has been reduced to about one-fourth of its bulk, a milky fluid with an appreciable deposit will result; the supernatant liquor may be poured off, and the residue thoroughly well washed in a large test-tube, and allowed to settle. This operation should be repeated several times, after which it will be found that the deposit consists almost entirely of the tests of *Foraminifera* mixed with a variety of things, such as sponge and other spicules, &c. Conspicuous amongst the *Foraminifera* two beautiful forms may be recognised; these are *Globigerina* and *Textularia*.

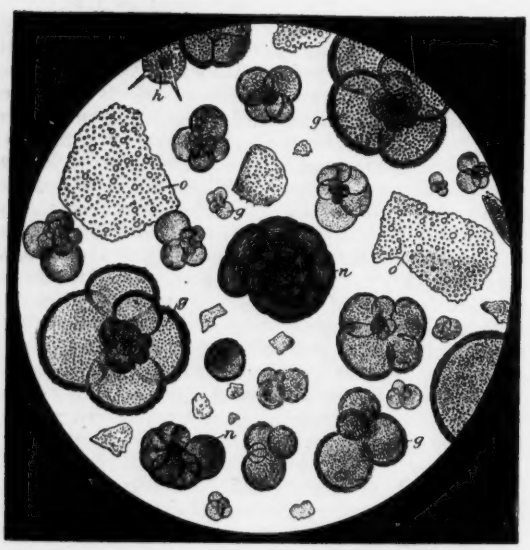


Fig. 3.—GLOBIGERINA OOZE, floor of the Atlantic Ocean, × 65. *g. Globigerina bulloides; o.* Fragments of *Orbulina universa*; *n.* Nautiloid Foraminifera; *h. Haliomma*, one of the *Polycystina* (*Original*).

As long ago as 1865-1866 Major Owen read two papers to the Linnæan Society, descriptive of the life-habits of *Globigerina* and other forms, which he obtained from the surface-waters of the Indian and Atlantic Oceans by the use of a tow-net. Later on, Gwyn Jeffreys* asserted that these were exclusively oceanic, and confined to the upper stratum of the sea, in opposition to the opinions of his associates, Wyville Thomson and Carpenter, that they were abyssal. During the voyage of the *Challenger* Murray had ample opportunities for proving the correctness of Jeffreys' statement, and showed beyond doubt that these creatures are indeed pelagic, and that only after death do their bodies subside to the bottom, where they form the basis of a whitish deposit termed "Globigerina ooze" (Fig. 3), so called from the preponderance therein of the tests of *Globigerina*.

We shall have occasion in future to point out that other forms than *Foraminifera* (*e.g., Radiolaria*) live, die, and are in like manner deposited. Fig. 4 shows two Radiolarians drawn from living specimens.

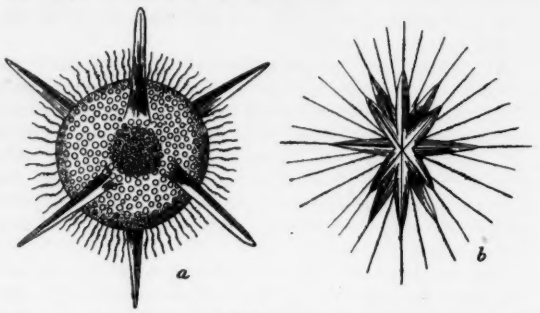


Fig. 4.—RADIOLARIA; *a. Haliomma hexacanthum*, one of the *Polycystina*, with radiating pseudopodia; *b. Acanthometra lanceolatal.* (After J. Müller, Abhandl. d. K. Akad., Berlin, 1858).

The shells of the fresh *Globigerinæ* are pellucid, and of a firm consistency; they are perforated by many pores, surrounded externally by a kind of crest, so that the pore commences at the bottom of an hexagonal pit; at each angle of this crest a delicate, long calcareous spine is given off, and these radiate from the centres of the respective chambers of the shell to which they belong. The whole system of spines produces a marvellously beautiful effect, and has been accurately figured and described by Wyville Thomson;* he has stated that pseudopodia have not been discovered in these animals.

After the tests sink to the bottom to produce the so-called ooze, they become opaque, lose their spines, and finally disintegrate.

In Fig. 3, which represents a sample of Globigernia ooze from the floor of the Atlantic, fragments of another organism may be observed. These are portions of the tests of *Orbulina universa*, and may be readily distinguished from those of *Globigerina* by their pores, which are of two sizes, one about four times as large as the other. *Orbulina* is spherical in shape, and sometimes contains a series of internal chambers, which resemble a small *Globigerina*. This led Max Schultze and others to suppose that it is but a reproductive phase of *Globigerina*.

It has been shown by the *Challenger* results that *Globigerinæ* exist in vast numbers all over the surface of the ocean, and are especially abundant in the warmer seas; accordingly, it would be but natural to suppose that the deposit on the sea-bed should always contain their tests; but this is by no means the case, and was at first an enigma to the naturalists of the *Challenger*, who came to a solution of the difficulty through the following observations:—They found that with increasing depth the nature of the deposit varied—so much so that they were able to predict what it would be at a given depth. At about 2,250 fathoms the Globigerina ooze gradually changes into a "grey ooze," which is characteristic of 2,400 fathoms. Deeper down (2,700 fathoms), the deposit is known as "red clay." From chemical experiments superadded to observed facts, they were led to discover that the calcareous Globigerina ooze is so acted upon by the solvent action of the sea water, that it is gradually broken down into the grey ooze, and

* Proc. Roy. Soc. London, Nov. 121, p. 443.

* Proc. Roy. Soc. London, 1874, No. 156, p. 35, Plate I.

thereafter into the red clay, which proved on analysis to consist of a silicate of the red oxide of iron and alumina, and which now contains but few testaceous remains (siliceous shells of *Radiolaria*, sponge spicules, &c.)

Sir Wyville Thomson noted the capture of numerous highly organised *Invertebrata* in this red clay, such as large *Holothuria* with only rudimentary calcareous neck-rings, and a paucity of or no spicules in their tegumentary system; also of delicate *Polyzoa* with their zooëcia almost wholly membranous, and the tubes of a tube-building annelid (*Myriochele*) still living; and he reflects on the possibility of such a formation in palæozoic times, the vestiges of which are to be found in the Cambrian *Oldhamia*, in siliceous sponges, and peculiar thin-shelled shrimps.*

The Globigerina ooze is usually bound together by a very fine comminuted paste, which at first sight seems to be amorphous, but under a high power of the microscope is resolved into a variety of rounded and excessively beautiful forms, two of which are here given in Fig. 5.

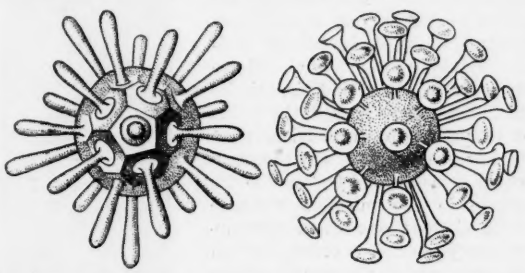


Fig. 5.—RHABDOLITHS from *Globigerina ooze*. (After Sir Wyville Thomson, Proc. Roy. Soc., Vol. xxiii., Pl. 3.)

They have been termed "rhabdoliths," and are of the nature of similar rounded bodies called "coccoliths" by Huxley, and supposed to be *Algæ* of a peculiar form.

In conclusion, the formations which are now going on at the bottom of the ocean, and which are chiefly composed of these minute animal remains, point conclusively to the manner in which extensive formations, such as the chalk, Nummulitic limestones, &c., were evolved, and to the importance of the living things, which, although they are so diminutive apart, and not of any great commercial value directly, are intensely interesting to the philosophical observer.

THOUGHT-READING.

"NONSENSE dies hard," says Mr. Labouchere, and he is perfectly right; but no nonsense dies harder than the nonsense of infatuated prejudice. Amongst literary men, those who know Mr. Henry Sidgwick and Mr. Labouchere—the two who wrote to the *Times* on the Thought-reading wager—very few, we suspect, would prefer Mr. Labouchere's judgment on a matter of evidence of this kind to Mr. Sidgwick's. Mr. Sidgwick has as cool and sceptical a temperament as Mr. Labouchere himself, but he has in addition a very much larger knowledge of the subject under investigation, and knows how absolutely childish it is to speak of such a power as some persons impute to Mr. Bishop as a miraculous and all but incredible thing. We say this without having formed any definite opinion ourselves on the subject of Mr. Bishop's powers, and, indeed, with a strong prejudice against a man who mixes up common conjurer's tricks with the professed attempt to illustrate obscure psychological powers of this nature.

But this we will say, that to all who have studied the subject, evidence literally *abounds* of the existence in rare cases of powers of thought-reading much more remarkable than any alleged in the case of Mr. Bishop. As we do not like to make this sort of statement without any kind of verification, we will take a modern instance from the writings of a Bristol medical man, Dr. Davey, who published a paper in the *Journal of Psychological Medicine* for April, 1881 (Part 1 of Volume VII. of the *Journal*), which records the case of a patient of his, investigated by him in concert with two other Bristol medical men—Dr. Andrews and Dr. Elliott. Here is Dr. Davey's description of Mrs. Croad's state:—

In 1870, it is stated, "she became totally blind;" in the following year deaf, and in 1874 speechless. The paralysis, which was limited to the lower extremities, involved, in 1879, the upper limbs; but at this time the loss of sensation and motion is limited to the left arm, the fingers and thumb of the left hand being but partially affected. The right hand and arm have recovered their once-lost functions. She is now able to articulate, though with difficulty, from, as it appears to me, a tetanic rigidity of the temporal and masseter muscles, by which the mouth is kept, to a large extent, fixed and closed. It was in October last [*i.e.*, October, 1880] that I was asked to see Mrs. Croad. I found her sitting on a semi-recumbent position on a small bedstead, her head and shoulders resting on pillows. The eyelids were fast closed, and the left arm and hand resting by the side. The knees I found then, as they are still, bent at an acute angle, the heels closely pressed to the under and upper parts of the thighs. . . . . Since October, and through the months of November and December, 1880, I have subjected Mrs. Croad to many and various tests with the view of satisfying myself as to the truth or otherwise of the statements given to the world of her blindness, sense of touch, and marvellous sympathies. To my near neighbours—Drs. Andrews and Elliot—I am much indebted. The various tests referred to were witnessed by them in my presence, and with the effect of assuring us that she (Mrs. Croad) was and is enabled to perceive, through the aid only of a touch, the various objects, both large and small, on any given card or photograph. After an experience extending over some nine or ten weeks, during which the "tests" were many times repeated, and, now and then, in the presence of several medical and non-medical (ladies and gentlemen) friends, there remained (I believe) not the least doubt of this "transference of sense" from the eyes of Mrs. Croad to her fingers and the palm of her right hand. It need not to be supposed that I and others were content to believe in Mrs. Croad's blindness, and to take no specific precautions against any possible trick or deception—far from this. On solicitation, she very kindly assented to be blindfolded, after a very decided fashion; and so blindfolded, that neither deception on her part nor prejudice nor false judgment on ours were—either the one or the other—possible. The blindfolding was accomplished thus: a pad of cotton wool being placed on each orbit; the face was then covered by a large and thickly-folded neckerchief; this was tied securely at the back part of the head, and—even more than this—more cotton wool was pushed up towards the eyes, on either side of the nose. Not content, however, the aid of two fingers of a bystander were called into requisition, and with these a continued pressure was kept up, during the 'testing,' outside and over the neckerchief and wool, and above the closed eyes. At this stage of the proceedings the room was, on two different occasions, very thoroughly darkened. Under such circumstances it was the testing commenced, and continued to the end; the result being, as theretofore, in the highest degree, conclusive and satisfactory. The transference of sense from one organ to another as an acquired and spontaneous condition of being must, on the evidence here adduced, be accepted as a demonstrated and certain fact. I would state here, that on receiving a picture card or a photo' from a bystander she (Mrs. Croad) places it on and about the chin or mouth, and perhaps draws it across the forehead, but the minute examination of the card is, apparently, the work of the fingers of the right hand. These several acts are, for the most part, followed by a quiet and intense thought, a well-marked concentration of mind on the picture, or whatever it may be, when, after a short time, she writes on a slate kept near her a description—sometimes a full and detailed one—of the card, its colouring, and the several objects thereon. I have seen some forty or fifty picture-cards and photographs described by Mrs. Croad at different times with various

* *Loco citato*, p. 47.

degrees of accuracy during the whole period I have known her. Occasionally her rapid and precise perception, or, if you prefer the word, conception, of the picture, and of the many, yet minute and trifling, objects going to form its entirety, is really startling. I have but seldom seen her wholly at fault, though she has met with her failures.

Now, this seems to us a much more marvellous power than that of thought-reading; but this is not all. Mrs. Croad appears to have had the very power which Mr. Bishop attributes to himself, in a very much higher degree:—

Sitting quietly by, or near to, Mrs. Croad, my attention has been again and again rivetted on the manner in which Miss Croad holds communion with her mother. Miss Croad does very certainly move her fingers over and about the face of her mother, but few, if any letters or words are formed by her. Watching her very narrowly, on several occasions, I felt at length assured that Miss Croad's communications were altogether unlike those made by either visitors or friends. The latter named formed letters, and with these words, and so conversed—if the expression be allowed—with Mrs. Croad; but it is not so with her daughter. Impressed with the fact as above stated, I spoke to Miss Croad of it, when she told me that as the rule it was requisite simply that she put herself in a close or personal contact with her mother to convey to her what was wished, or to give her a knowledge of this or that, as the case may be. Now, so marked a mental sympathy or concordance as this is altogether without or outside the experience of most of us; and it is therefore well worthy the attention of those present who have the courage to investigate, what I may well call, unorthodox medicine. . . . . As a further illustration of Mrs. Croad's peculiar and clairvoyant gifts, it should be stated that at my second interview with Mrs. Croad, and in the presence of Dr. Andrews and others, certain of my own personal and private convictions on a particular subject became, as it would seem, in a strange and exceptional manner, known to Mrs. Croad. She asked me if I would allow her to tell me a secret in my own life-history, and would I be offended if she wrote it on her slate. I replied, "No." That written on the slate was and is a fact, than which nothing could or can be more truthful and to the point. Dr. Andrews is prepared to verify this; the others present on this occasion were but little known to me.

**Here we have one of the most remarkable amongst numbers of instances of thought-reading, known to all students of the more abnormal facts of psychology—an instance encountered by steady-going professional men, in the ordinary course of their profession, and never produced on platforms for the amusement of the crowd at all. Dr. Carpenter, in his remarkable work on "Mental Physiology," has admitted the probability of the existence of some such power as this, on the evidence in his own possession; indeed, Mr. Bishop declares that Dr. Carpenter has verified the real existence of some kind and degree of this power in Mr. Bishop himself, and has stated his belief that Mr. Bishop's powers have been tested under strictly scientific conditions. Now, we do not pretend to have any specific opinion of our own upon Mr. Bishop's case, and have absolutely no right to any such opinion. Nothing is more marvellous than the assumption of a mere man of the world like Mr. Labouchere, that because the phenomena have never come within his knowledge, they are incredible. To him, apparently, opinions like Dr. Carpenter's are not even entitled to a respectful recognition, for he does not refer to them, unless it be in the remark that "nonsense dies hard." At all events incredulity dies hard. There are plenty of facts on which eminent medical men have come without having any motive whatever for credulity, and to which they have been compelled to give their attestation, such, for instance, as those we have quoted from Dr. Davey's address to the Bath and Bristol Branch of the Medical Association, far more remarkable, and far more difficult to bring under any of the known laws of nature, than the achievements of Mr. Bishop, even if these achievements be what Colonel Statham and Colonel Trench affirm, and what Mr. Labouchere denies.—*Spectator*

FLIGHT OF A VERTICAL MISSILE.

BY RICHARD A. PROCTOR.

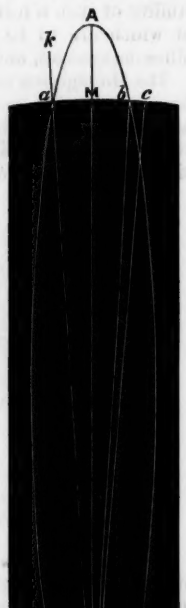
Let a b c *be part of the earth's equator,* C *the earth's centre, and let a missile be projected in direction* a k *(vertical), with a velocity of* 1,000 *ft. per second. Supposing the point* a *to be carried to* c *during the flight of the missile, determine the point,* b, *where the missile will reach the ground—in other words, determine* b c *the westerly deviation of the missile.* (*The resistance of the air is neglected.*)

Let aAb be the actual path of the missile, and part of a long ellipse AA′ about C as farther focus, the portion aAb being appreciably parabolic. Join Ca, Cb, Cc, and draw AM to bisection of arc ab. Then, by Kepler's second law, the missile in moving around the path aAb, sweeps out equal areas around C; and obviously the point a sweeps out equal areas around C, as it is carried uniformly along the arc abc. Moreover, it is clear that the missile as it starts from a and the point a in passing from its initial position a are sweeping out the same equal areas around C (for the increase in the missile's distance from C does not affect the momentary description of areas, which depends only on the rate of motion perpendicular to the radius vector). This equality continues throughout the motion, since neither the missile nor the point a changes its rate of describing areas around C. Consequently, when the missile reaches b, at which time the point a has come to c, the total areas described are equal, that is,

area $aCbA$ = sector aCc, or, removing common sector aCb

area aAb = sector bCc,

that is, since aAb is appreciably a parabola and aMb a straight line perpendicular to the axis AM,



$$\frac{2}{3}ab.\,\mathrm{AM} = \frac{1}{2}bc.\,ab\mathrm{C}.$$

$$\therefore \frac{bc}{ab} = \frac{4\mathrm{AM}}{3b\mathrm{C}};\ \text{or, putting AM}=h, b\mathrm{C}=r,$$

$$\text{westerly deviation} = bc = \frac{4}{3}\frac{h}{r}.ab.$$

Compared with ab, bc is so small that we may put ac for ab without appreciably affecting the result; and obviously, if $2t$ is the time of flight (t for the ascent or descent), P the earth's rotation period, we have

$$ac = \frac{2t}{\mathrm{P}}.2\pi r$$

$$\text{wherefore } bc = \frac{2t}{\mathrm{P}}.2\pi\left(\frac{4h}{3}\right) = \frac{16\pi ht}{3\mathrm{P}}$$

Cor.—If the place of projection is in latitude λ, the westerly deviation is

$$\frac{16\pi ht\cos\lambda}{3\mathrm{P}}.$$

Note.—I said incorrectly that Mr. Bray's result at p. 393, letter 853, was correct. It is only half the actual

deviation. For, if we substitute in the above formula his correctly stated values for t and h, we get westerly deviation for a missile projected vertically with a velocity of 1,000 ft. per second

$$= \frac{16 \times 3000000 \times 6000 \times \pi}{3 \times 193 \times 193 \times 24 \times 60 \times 60}$$

$$= \frac{1000000\pi}{(193)2 \times 9} = 93{\cdot}62 \text{ ft. (about).}$$

It may interest inquirers into problems of this sort to find out why Mr. Bray's apparently correct reasoning leads to a result twice as great as it should be. If I can find time and space, I will give, shortly, in the mathematical column, an analytical solution of the problem.

GOD'S WILL.*

SPEAKING of the Sunderland accident, the *Spectator* makes the following pregnant remarks :—

"The moment the calamity was known, every one seems have behaved admirably, and the tone of the town since has been the true one, that of sad but helpful resignation to an inexplicable Will. For it is an inexplicable Will, especially to those who believe, as we do, that God governs, as well as reigns. What should we say of a man who, merely by putting a thought into Mr. Fay's head, the thought to stand at the gallery-door and see the children out in batches, could have prevented that ghastly massacre, and did not put it? Yet that must be true of the Almighty, if any one of our ideas about his attributes is true, if he foresees, if he is all-powerful, if he has free-will. Apart from the suffering—suffering often to the good, for it was probably the kindest parents who sent their children to the show—why does he allow all that monstrous waste of life among the innocent, that destruction of potential usefulness? The only answer is the simple and unsatisfying one that we know nothing about the matter, and never shall know all, though we may know much more than we do now. Man cannot know the policy of God, which is not shown as changed, but as always the same, in these great catastrophes. They do but concentrate a process which never stops. Taking Asia and Europe together, the half of all children born die before they are two. It is certain that more children died in London in the week of the catastrophe, unnoticed, than died in Sunderland to the horror and pity of the world. It is quite probable, though there are no statistics, that more children died in the United Kingdom still-born on Saturday than died in that staircase shambles. Vast, unending, inexplicable waste of life, never utilised even for a little while, is the law of the planet, the will of its Creator, as little to be made intelligible by thought as the endless mystery of non-educative pain. Theology gives us no more light on the subject than Science does, and though it is best, or at least most comforting, to think that the children are the happier for escaping this life and its miseries, there is no proof of that—rather evidence from analogy that they lose an opportunity which would have been to their profit. Why not, if the world, and life in the world, are, as we all suppose, of any use at all? Men are not the better for escaping, but for fulfilling, duties. There is no explanation whatever to be found, nor is there any need of one. If man acknowledges God at all, he must acknowledge a Being whose wisdom must be so far above his own that failure to perceive it is failure in the creature, not in the Creator. It may not be an absurdity, though it seems one to us, to recognise God, and yet think that he can err; but it is certainly a folly to think that we can detect error in Him. The theologian, like every other man, is studying the Infinite, and when he has thought himself out, he can only acknowledge that he is always at last face to face with a mystery past his solution. Thought sometimes only helps us to pile up more figures on the recurrent decimal.

* From the *Spectator*.

Editorial Gossip.

ONE cannot but be amused at the tone of jeering superiority with which the psychical problems involved in such questions as thought-reading and thought-finding are dismissed by folk who are only not stupidly credulous because they are stupidly incredulous. With such persons there is no middle path; they either refuse to consider evidence at all, or they swallow everything they are told. In either case it is the same defect in the unscientific mind which does the mischief. A man of this class believes or disbelieves—he does not inquire. Take such a subject as mesmerism, for example:—"Mesmerism! my good sir," he will say; "mesmerism is utter humbug; nothing in it but lies and trickery;" or, it may be (just according to the way in which he has taken up the matter), he will talk equal nonsense on the other side, and tell you that mesmerism is the most marvellous power in existence.

MANY years have passed since I made my first acquaintance with these two classes of persons—or rather with these two forms of the same class. A man named Zamoiski came to Cambridge, who professed to possess marvellous mesmeric powers. He really had a very fair amount of that particular mind-influencing power which we may conveniently call mesmerism (as a name not suggesting any theory as to the nature of the power). But he promised all manner of performances, far outside of anything he or any man could do without the aid of confederates. He appeared on the platform, went through a number of performances, some interesting and curious, but by no means miraculous, on college men of various standing, others which would have been marvellous indeed if they had been genuine. But they were performed on certain boys who were no doubt paid for their parts in the display.

I FOUND after the performance that those who had seen it could be divided into three classes, two nearly equal in numbers and one much less than the others. There were a number who supposed the whole series to be humbug, the collegians being in the trick like the rest; a number more accepted the whole series as genuine because they *knew* that the collegians were not in it. The few, more scientifically minded, recognised the evidence for what it was worth. A certain phenomenon, partly mental, partly physical, had been observed, strange enough to explain why weak-minded persons supposed there was something uncanny in it, but not at all outside what is natural. This phenomenon, though rejected by the weak-minded of another sort as sheer trickery, seemed yet well worth inquiring into in a scientific way,—that is, under the test of observation and experiment.

I SUPPOSE I had begun, even at that time, to have scientific tastes—though my friends and the worthy Fellows who had dealings with me failed to recognise this characteristic. Any how, I determined to test Zamoiski's powers by enabling him to give a less public entertainment. My largest sitting-room (I occupied rooms intended for a Fellow) could accommodate more than forty. So I and a

few friends invited the mesmerist to try his powers on as many as could be brought together, at a day's notice, in my room. There came, in all, forty-two, including seven or eight "dons" (among them the Rev. S. Parkinson, my private tutor), about a score of Wranglers, and ten or twelve first-class Classics—"in sooth a goodly company." Zamoiski came along in due time, with a galvanic apparatus to keep him lively. He wanted to go through his customary discourse; but "we would none of it." We wanted simply to test his power of influencing mind and muscle. He tried nearly all of us (all, in fact, who were not unwilling). Among the number was my friend, W. Baily (Second Wrangler that year), who had been influenced to some degree—slight, but enough to show the reality of the phenomenon at the public exhibition—and Mr. Garrett, our organist (collegian also), who had felt the influence of Dove and other mesmerists. Zamoiski failed all round at the first trial. On this Mr. Garrett said: "You ought to succeed with me; for Dove did," or words to that effect. Zamoiski then gave his whole energies to influencing him, and at last with such success as to make Mr. Garrett forget (or rather become for a few moments unable to recall) his own name. After this he tried others with whom he had before failed, but in whom he had recognised signs of yielding to the mesmeric influence (whatever it may be). He achieved a fair measure of success. My friend Baily was so far influenced as to be unable to open his eyes except with an effort. Others were unable to strike the mesmerist's hand, though they went willingly—even rather viciously—for it. (This had happened in the public exhibition with well-known University men, some of whom, as Ingham of the 'Varsity boat, would have made M. Zamoiski remember the evening, if they had got in at his rather soft-looking hand with their clenched fists, as they tried hard to do.) One or two had to struggle to remember their names, but succeeded after a few seconds. As I had expected, nothing very wonderful was done, but every experiment was satisfactory, as the whole company were known to each other, and all anxious to put Zamoiski to the test. Several who were not actually influenced said they felt they had to exert an active resisting power. I was not one of these. He tried his power on me in public and in my rooms, and I noticed only that a sort of resistance was excited which was barely noticeable, so easy it seemed to overcome the influence he was trying to exert. Even this I only noticed when, of set purpose, I sought to yield my mind to any influence which might be exerted upon it by him.

A few days later my fencing-master, Jackson, a sixteen-stone man, of splendid muscular development and full of vitality, told me that, while showing what Zamoiski had done and tried to do, he had found that he himself possessed the same power, and in rather greater degree. Zamoiski was a fat man, but not muscular. He seemed to be full of coarse vitality; but his lecturing (save the mark!) did not suggest the idea of superabundant mental power.

The phenomena which the Pyschical Society are endeavouring to investigate are akin to those called mesmeric in several respects. There is nothing supernatural about them, though much that is mysterious, in the same sense that (for example) gravity is mysterious (but in infinitely less degree). Of course, to unscientific minds they necessarily appear either miraculous on the one hand, or imaginable on the other. Fortunately, it is not from either class of mind that science expects any developments in the way of progress. The most brilliant idea which has yet occurred to *such* minds has been that of proffering a wager!

Of Mr. Bishop personally, I may say, *vidi tantum*. Like his fellow-countryman, the late Dr. Beard (a thoroughly scientific inquirer, almost equally abused by the would-be keen ones), he appeared to me a man of an intensely nervous temperament. I know, from the report of trusted friends, that his powers of thought-reading and feeling-reading are very great; and in the range of such powers in what he claims to do and to have done there is certainly nothing beyond the natural. That he should obtain money through the exercise of his gifts does not seem to me open to exception, any more than that Bishops of another sort should receive money for exercising spiritual gifts—a point about which a certain Jewish writer has made very sensible remarks. It is unfortunate, perhaps, that so few take interest in purely scientific research that Mr. Bishop has had (I suppose) no choice but to relieve his mind-experiments with conjuring tricks; but in this there is nothing more objectionable than there is in the lecturer on astronomy relieving the strained attention of his hearers by an occasional anecdote or jest. The cool assumption that, because the power claimed by Mr. Bishop is beyond *their* slow minds, dullards are free to treat him as if he were a proved swindler, would be discreditable to them in the extreme, if it were not that—presumably—they know no better.

A Cheap and Good Telescope.—We have received from Mr. Theobald one of his singularly cheap telescopes, which we have had excellent opportunities of testing during the last few weeks at the seaside. It is an excellent instrument—a perfect marvel for the price. It comes sharply and cleanly into focus, has a uniform field, and gives clear and good definition. We have not yet tried it on many double stars, not having a suitable stand. But it has done well with those we have tried. This little telescope will be found to give very pleasing miniatures of the lunar views described by F.R.A.S., though, of course, the young observer must not expect it to show such details as a 3-inch telescope presents. We hope soon to test the work of this capital little telescope on Jupiter and Saturn, and will report the results in due course.

Powder Magazine Fired by Lightning.—A telegram from Scutari, Albania, dated 8th ult., says:—"The powder magazine of the fortress of Scutari was struck to-day by lightning. A great explosion followed, and a portion of the bazaar was destroyed. Many persons are reported killed." Later information says that ten soldiers and seven civilians were killed, and forty soldiers and fifteen civilians injured.

The Forth Bridge.—On Thursday, the 14th inst., Sir Thomas Tancred, one of the contractors for the Forth Bridge, laid, with Masonic honours, the first granite block of stone in connection with the structure. He was accompanied by Mr. Symons, one of the Government Inspectors, Mr. Gray, manager of the works at North Queensferry, and several other gentlemen. The stone, of pure Aberdeen granite, weighing about 16 cwt., was placed at the south-west corner of pier No. 12 from the south end of the bridge, which is situated a little to the east of the church at North Queensferry. There are now at the works—north and south—nearly 100 tons of Aberdeen granite for the outer casing of the piers. Four steam stone-crushers, each capable of crushing 80 to 90 tons per day, are at work preparing the whinstone for centring of the piers, and all along the line the greatest activity prevails in getting ready the preparatory operations required for this great structure.

THE FACE OF THE SKY.

FROM JULY 6 TO JULY 20.

BY F.R.A.S.

THE sun will be watched, as usual, for groups of spots and faculæ, which have recently become both more numerous and interesting. The face of the night sky will be found depicted on Map VII. of "The Stars in their Seasons," but there is no real night in the United Kingdom until July 22. The heavens continue to offer a practical blank to the observer, as far as the apparition of any of the planets is concerned. The moon is 1·9 day old at noon to-day (July 6), and quite obviously 15·9 days old at the same hour on the 20th; hence she is favourably placed for the observer during the greater part of the period of which we are treating. Her south declination, however, becomes considerable during the latter half of it. Two occultations of stars occur during the next fourteen days. The first is that of the sixth magnitude star 28 Libræ, which will disappear at the moon's dark limb at 11 h. 11 m. p.m. on July 14, at an angle of 119° from her vertex; but she will have set ere it reappears. The second occultation occurs on July 17, and is of B. A. C. 6,081 (also a star of the sixth magnitude). This will disappear at the dark limb of the moon at 11 h. 31 m. p.m., at an angle of 133° from her vertex; and reappear at her bright limb at 12 h. 41 m. p.m. at a vertical angle of 277°. During the next twenty-four hours the moon travels from Cancer into Leo. There she remains on the 7th, passing into Sextans during the early morning of the 8th. She leaves Sextans and travels into Leo again about 3 a.m. on the 9th. She remains in Leo until about 6 a.m. on the 10th, when she moves into Virgo. She does not leave this constellation until about four o'clock in the afternoon of the 13th, when she crosses into Libra. There she continues until about 6 p.m. on the 15th. In the course of the next ten hours she travels across the northern part of Scorpio into the southern portion of Ophiuchus, where she continues until six o'clock in the evening of the 17th, when she enters Sagittarius. She remains in Sagittarius until 8 a.m. on the 19th, and by noon on the 20th has travelled into Aquarius, where we leave her.

"Let Knowledge grow from more to more."—ALFRED TENNYSON.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

All Editorial communications should be addressed to the EDITOR OF KNOWLEDGE; *all Business communications to the* PUBLISHERS, *at the Office, 74, Great Queen-street, W.C.* IF THIS IS NOT ATTENDED TO, DELAYS ARISE FOR WHICH THE EDITOR IS NOT RESPONSIBLE.

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NO COMMUNICATIONS ARE ANSWERED BY POST, EVEN THOUGH STAMPED AND DIRECTED ENVELOPE BE ENCLOSED.

RATIONAL DRESS.

[864]—The female members of my family complain that "Pedestrienne" fails to describe the only part of her dress which they want to know how to make, viz., the "divided skirt." They would like to try the costume recommended, but ask for a detailed description of what "Pedestrienne" means by a "divided skirt."

CYCLIST.

GENEALOGICAL PUZZLE.

[865]—The following may amuse some of your younger readers:—A and B are two persons in no way related—of no consanguinity. They meet, and one asks the other:—"How is our brother Tom?" [Tom being brother to both of them.] Show how this may be.

J. M.

LETTERS RECEIVED AND SHORT ANSWERS.

J. R. (Malta).—As to "Stars in their Seasons," note advertisements.—MORE LIGHT. Yes; if your facts are "firmley established," you can—as you say—"afford calmley to wait." (You seem, indeed, to take your e's pretty freely.) I really cannot say who is "the best living authority on the subject;" not being quite clear what subject you mean; you only mention your "facts of the weather," which you apprehend I "may think nonsencical" (here you are clearly "half c's over"), "and therefore worthless" (which, if nonsensical, they probably would be); but I don't know what they are. However, on nearly all subjects there is variety of opinion as to who is the greatest authority. As you say, "some old one man and some another."—G. MACKINLAY. Thanks. Both statements relate to cannon balls flying athwart the range of vision, though this is not expressly stated. In mid-flight, which was what I referred to, a cannon ball has not a greater rate than 500 yards per second, even when its initial velocity has been as great as 900 yards per second. I have myself frequently seen cannon balls in mid-flight from behind the gun.—A. PARNELL. Many thanks; an occasional article might find insertion; but, as you may have noticed, space is crowded. The subject is full of interest.—A LEARNER. Six inches square = 3 in. by 1 ft.; that is what was meant.—W. B. No; the question whether a wager is won or not is quite out of our line. Of the wager in question, one can only say that it was vulgar to offer it, unwise to accept it. Science has nothing to do with wagering, except to say that all forms of gambling are essentially immoral.—T. J. DEWAR. Your manner is so rough (not to say rude) that though your matter has been in part useful, it is scarcely worth getting at through such breakers. Hereafter, I fear your handwriting (or signature, if handwriting not recognised) will cause our trusty sub-editor to "basket" your communications rather summarily. If a passer-by flings a stone through my drawing-room window, he would probably be removed by A 21 before I had learned from him that he only wanted to give me his opinion about the kitchen chimneys. I trust I make myself understood. You fling "awful rubbish" at my head or that of a valued contributor—'tis the same thing: do you suppose I go beyond that to read your detailed criticisms?—E. A. HOBSON. Many thanks; the Indian observatory most interesting. There is a description of it in one of the earlier volumes of the *Penny Magazine*.—IKE. (1) The position of the earth's axis varies sensibly (to instrumental tests) from summer to winter, through the effects of precession and nutation. (2) Try "York's List of Astronomical Diagrams." (3) See advertised list of lectures. (4) Algebra certainly essential.—H. MALIM. Calculation given this week.—A WORTHY CORRESPONDENT. Many thanks.—TIM J. W. Cannot answer by post.—G. DUNCAN. Thanks; your communication submitted to the proper persons to decide about such matters.—G. JORDAN. The movements of the moon, as seen from all latitudes, north and south, have been in precise accordance with accepted theories. Why should the moon *not* be seen every month in the Arctic regions, seeing that every month the moon is half the time north of the celestial equator?—J. M. Thanks. Puzzle not very difficult. In my own family "our brother" appears in another way. Thus, if widower having a child marries a widow who also has a child, a son by this marriage is for his child and hers "our brother," though they are in no way related by blood.—ZOOLOGIST. Have no idea.—MARY, HOUSEMAID. Not bad, my dear sir, not bad; nay, "the stars and their seasonings" very good—rather *too* good for "Mary, Housemaid." The "combination" [of] accidents impossible. N.B. Anyone who would spell "minute" with two "n's," would spell it with two "i's," *not* "minnute." Both *my* "i's" open so that I C through U with E's.—S. TRURO. See solution this week.—G. W. Really we do not know what you ought to do under the dismal circumstances you mention. When "you start a meal you scarcely know when to finish;" no wonder that at night you "feel weary, and also your eyes are heavy too." The best advice we can give you about over-eating is "Don't." But we are not medical.—W. FITZROY. We do not know how your namesake acted; and the question is not one we should like to ventilate here.—EQUITES. Really, some readers ask curious questions. You say that when riding, the sides of your knees get sore, and you ask how to avoid that trouble. Keep on and it will go off. Besides, its the best way to keep on. If this fails, try riding with the *other* sides of your knees towards the horse; you have no idea how effective that will be (and also look). But why do you ask *me* this particular question? Now, if you were to try the editor of *Nature*, who compares the motion of the earth to the motion of a galloping horse on a smooth plain, of which motion the rider remains—he says—unconscious, you might learn how to save your knees. I have never reached this stage in riding, but still remain emphatically conscious of motion during a gallop, even over the smoothest fields.

SUB-EDITORIAL.

K. L. W., PERPLEXED, J. HAWKINS, M. E. W., H. N. MEDLICOTT, K. R. and N. M., LANGUAGE, HALLOWEEN, and others. Unsuitable—T. J. DEWAR (later communication). See editorial remarks.

Our Mathematical Column.

GEOMETRICAL PROBLEMS.

By RICHARD A. PROCTOR.

PART VI.

LET us next try a few problems—properly so termed—that is, propositions in which something is required to be done. In these, as we have said, the analytical method is nearly always to be preferred. We will begin with a simple example.

Ex. 6.—*On a given straight line describe an isosceles triangle, each of whose equal sides shall be double of the base.*

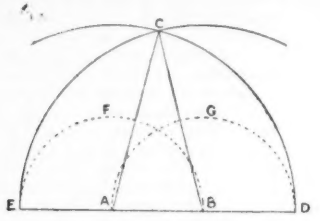


Fig. 11.

Let A B (Fig. 11) be the given straight line.

Suppose that what is required is done, and that on the base, A B, there has been described the triangle, A C B, in which the sides A C and C B are equal to each other, and each double of the base A B; and let us consider what constructions are suggested.

It seems hardly possible that the resemblance between this problem and Euc. I., 1, should escape the student's notice. He will inquire, then, whether the method of that problem cannot be applied to the present one. Instead of the circle with radius equal to A B, we now require circles with radius equal to twice A B. It is clear, then, that if we produce A B to D, making B D equal to A B, and B A to E, making A E equal to A B (Euc. I., 3), then A D and B E will each be double of A B.* Therefore, if with centre A and radius A D we describe a circle D C F, and with centre B and radius B E the circle E C G, then C, the intersection of these circles, is the vertex of the required triangle. For A C and B C are severally equal to A D and E B—that is, are double of the base, A B.

We will next try the following:—

Ex. 7.—*The point P, Fig.* 12, *is within the acute angle formed by the lines A B and A C. It is required to draw through P a straight line which shall cut off equal parts from A B and A C.*

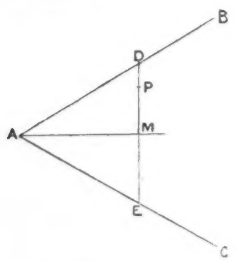


Fig. 12.

Let D P E be the required line, so that A D is equal to A E.†

Then D A E is an isosceles triangle, and it is an obvious course to see whether any of the properties of isosceles triangles will help us to a solution of our problem. Now, the only property of isosceles triangles explicitly contained in Euclid is that of Bk. I., prop. 5. This gives us the angle A D E equal to the angle A E D,—a property which avails us nothing.

But there are other properties of isosceles triangles, not expressly mentioned by Euclid, which every geometrician ought to be acquainted with. We will assume that the student is familiar with them—and indeed they are nearly self-evident. They are included in the statement that the perpendicular from the vertex on the base of an isosceles triangle bisects the base and also the vertical angle. Draw A M perpendicular to *the assumed line* D E; then the angle M A D is equal to the angle M A E, and also D M is equal to M E.

Now let us consider whether this construction affords us any hints:—

First, we cannot see how to draw the line through A perpendicular to *the real line* D E, because it is this very line we seek to draw.

Secondly, we cannot, for a similar reason, see how to draw the line from A to the bisection of D E.

But, thirdly, we *can* draw the line A M, bi-secting the angle D A E.

And this clearly gives us the solution of our problem, since we can now draw D P E at right angles to A M. Thus the solution runs as follows:—

Draw A M bisecting the angle D A E, and through P draw D P M E at right angles to A M; then shall A D be equal to A E. For, in the triangles M A D, M A E, the angle M A D is equal to the angle M A E, the right angle A M D is equal to the right angle A M E, and A M is common to the two triangles; therefore the triangles are equal in all respects (Euc. I., 26), and A D is equal to A E.

The proof of the equality of the triangles M A D and M A E was not included in the prior examination of the problem, since is involved in the assumed knowledge on the student's part of the fundamental properties of isosceles triangles, proved farther on. But, of course, it is well (in a case of such simplicity) to introduce the proof into the solution of the problem.

Let us next try the following problem:—

Ex. 8.—*The points P and Q, Fig.* 13, *are on the same side of the line, A B. It is required to determine a point C in A B, such that the lines P C Q C may make equal angles with A B.*

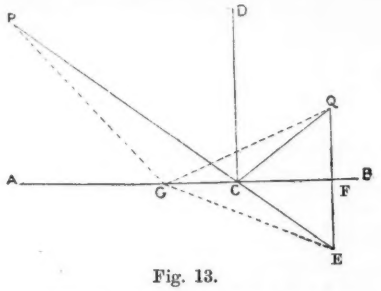


Fig. 13.

Let C be the required point, so that the angle P C A is equal the angle Q C B.*

Let us try drawing a line, C D, at right angles to A B. Then the angle P C D is equal to the angle Q C D. On a consideration of this relation, however, it seems unlikely to help us. For it is not easier to gather anything from the equality of P C D and Q C D, than to make use of the equality of P C A and Q C B.

It seems an obvious resource, since the equality of the angles, P C A and Q C B, as they stand, is not readily applicable to our purposes, to produce either P C or Q C, in order to see whether the vertical angle either of P C A or Q C B might be more serviceable to us. Produce P C to E. Then the angles Q C B and B C E are equal, or C B is the bisector of the angle Q C E. The only property connected with the bisector of an angle which seems likely to help us is this one, that the bisector of the vertical angle of an isosceles triangle is perpendicular to and bisects the base. Now, we can make an isosceles triangle of which C shall be the vertex and C Q a side, for we have only to take C E equal to C Q, and to join Q E, cutting C B in F. Then, by the property just mentioned, Q E is at right angles to C F, and is bisected in F.

These relations obviously supply all we want. For, reversing our processes, we have only to draw Q F E perpendicular to A B, and

---

* We have seen this problem given with the proviso that no problem beyond Euc. I., 1, shall be made use of. In this case the student will see at once that if, with centres A and B, and distance A B, he describes the circles B F E, A G D, then E B and A D, the diameters of these equal circles, are severally double of A B.

† In constructing the figure, proceed thus:—Take A D equal to A E, and join D E; then *take P, a point dividing D E into unequal parts.*

* Construct as follows: Draw A B, and from any point C in A B, draw the *unequal* lines C P, C Q equally inclined to A B. Then there is no risk that accidental relations will appear as necessary ones.

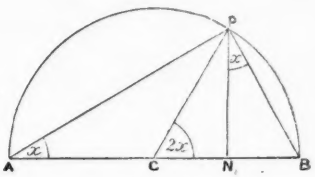
to take F E equal to Q F; then drawing P E to cut A B in C, we are certain that C is the required point. In *all* such cases we should not be equally certain that the *proof* would be as simple as the analysis, since sometimes the reversal of a process involves properties not so readily seen as their converse theorems. In this case, however, it is obvious (or will at least appear on a moment's inquiry), that the proof is simple.

For, join C Q (we are going now through the synthetic treatment of the problem, and therefore ignore the prior constructions). Then, because Q F is equal to F E, and C F is common and at right angles to Q E, the triangles C F Q and C F E, are equal in all respects. Therefore, the angle Q C F is equal to the angle E C F. But E C F is equal to the vertical angle P C A. Therefore the angle Q C F is equal to the angle P C A.

It is an excellent practice, when a problem has been solved, to notice results which flow from, or are in any way connected with, our treatment of the problem. In Ex. 8, Fig. 13, we notice that the line C Q is equal to the line C E, so that the sum of the lines P C, C Q, is equal to the line P E. It might occur to us to inquire what is the sum of lines drawn from P and Q to any other point, as G, in A B. Join P Q and Q G. Then the fact that C E is equal to C Q reminds us that if we join G E, G E will be equal to G Q. Thus P G and G Q are together equal to P G and G E together. But P G and G E are together greater than P E; that is, P G and G Q are together greater than P C and C Q together, or *P C Q is the shortest path from P to Q, subject to the condition that a point of the path shall lie on A B.*

ERRATUM.—On p. 397, 1st col., lines 17 and 18 from bottom, for "a right-angled triangle" read "an isosceles triangle."

GEOMETRICAL PROOF OF THE FORMULÆ FOR DOUBLE ANGLES.



Let A P B be a semicircle, with centre C.
P C N any angle less than 90° ($2x$)

$$\therefore P A N = x.$$

$$B P N = 90° - P B N = P A N = x.$$

$$\text{Sin } 2x = \frac{P N}{\text{rad.}}$$

$$= \frac{2 P N}{2 \text{ rad}} = \frac{2 P N}{A B}$$

$$= 2 \frac{P N . A P}{A P . A B}$$

$$= 2 \text{ Sin } x \text{ Cos } x.$$

$$\text{Cos } 2x = \frac{C N}{\text{rad}}$$

$$= \frac{2 C N}{2 \text{ rad}} = \frac{C N + C N}{A B}$$

$$= \frac{A N - \text{rad} + \text{rad} - N B}{A B}$$

$$= \frac{A N . A P}{A P . A B} - \frac{N B . P B}{P B . A B}$$

$$= \text{Cos}^2 x - \text{Sin}^2 x.$$

$$\text{Tan } 2x = \frac{P N}{C N} = \frac{2 P N}{2 C N} = \frac{2 P N}{C N + C N}$$

$$= \frac{2 P N}{A N - \text{rad} + \text{rad} - N B}$$

$$= \frac{2 \frac{P N}{A N}}{\frac{A N}{A N} - \frac{N B}{P N} \frac{P N}{A N}}$$

$$= \frac{2 \tan x}{1 - \tan^2 x}.$$

A. D. C. (Cantab.)

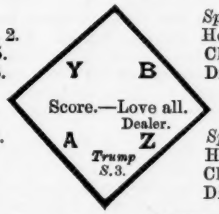
Our Whist Column.

BY "FIVE OF CLUBS."

THE HANDS.

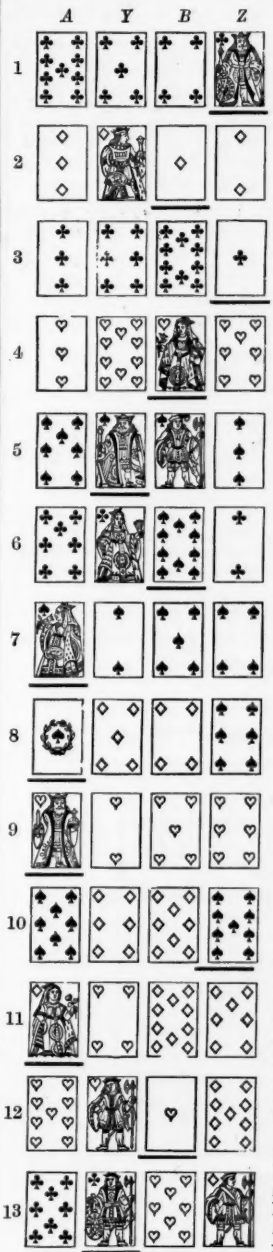
Y.
Spades—K, 2.
Hearts—Kn, 10, 4, 2.
Clubs—Q, Kn, 6, 5.
Diamonds—K, 6, 5.

B.
Spades—Kn, 10, 5.
Hearts—A, Q, 8, 5.
Clubs—10, 4.
Diamonds—A, 10, 8, 4.



A.
Spades—A, Q, 8, 7.
Hearts—Q, 9, 3.
Clubs—9, 8, 7, 3.
Diamonds—Q, 3.

Z.
Spades—9, 6, 4, 3.
Hearts—7, 6.
Clubs—A, K, 2.
Diamonds—Kn, 9, 7, 2.



NOTES ON THE PLAY.

NOTE.—The card underlined wins the trick, and card below leads next round.

1. The score being at love, *A* would have been justified, we think, in leading a trump, notwithstanding the weakness of his hand outside trumps. Still there are valid reasons for opening his longest suit; and on the whole, it was, perhaps, the safer course. With *A*'s cards in Clubs, too, it was not unlikely that the suit would be established at the third round. *A* knows that the Two is with *Z*, unless *Y* or *B* is signalling.

2. *Z* opens his long suit. *Y* does not hold the Queen.

3. *B* should have led his own suit, notwithstanding the major tenace. He knows from his partner's lead, himself holding Club Ten, that his Hearts are stronger than his partner's Clubs; and apart from that, the suit is intrinsically so strong that it was his duty to show it. But there are players who never will lead from a suit headed by Ace, Queen. The Club Two is seen now to be with *Z*, Eight and Seven with *A*; Queen and Knave with *Y*.

4. *Z* (Mr. J. Clay) has been blamed for discontinuing here his own suit. But there were good reasons; although the event turned out unfavourably. He knew that the Diamond Queen was held by either *A* or *B*. If by *A*, then a Club lead from *A* would give *B* a ruff, likely enough to suit him, as *Z* holds four trumps himself. If *B* holds the Queen, it is likely that, having no Clubs, he holds one or two more Diamonds, and that *A* holds no more. In this case there is danger of a cross ruff. On the other hand, as *A* led from a weak suit of Clubs, and *B* did not lead Hearts, it looked more than likely that both were weaker still in Hearts, and therefore that *Y* held great strength in that suit. On the whole, though the question is one of probabilities only, Mr. Clay seems to us to have shown his usual judgment in leading what he thought would be his partner's suit.

5. Having made his tenace, *B* might *now*, at least, have enlightened his partner as to the constitution of his hand. If he had led Ace of Hearts, *Z* playing the Six would have showed he had no more. Another lead of Hearts

would either have forced *Z*, or if, as is probable, *Z* had refrained from ruffing, *A*'s King would have made. Then a Club lead from *A* would have given *B* a ruff; and a final Heart lead from *B* would have given *A* a ruff. A small trump lead from *A* would have given *B* a trick; the next trick would have been made by *A*'s Ace, drawing the King; then the Queen would have made; and the game would have been won. And though it is not sufficient to point thus to results, we think there can be little room for doubt that the game should have been continued thus. But, like many other players, *B*, who would not lead from a long suit headed by major tenace, would not play from a suit led by an opponent, though it was clear the opponent had led from weakness.

6. *B* should have ruffed with the Five, Club Two being certainly with *Z*.

7. From this point to the end the play is very pretty. *A* can now place almost every card. He knows that *Z* has the winning trump and three Diamonds, probably headed by Knave. He knows that *B* holds the Heart Ace, *Y* the Heart Knave (for *Z* can have no more), and trick 4 shows the Knave is not with *B*. The Knave of Clubs is with *Y*.

10. Leading the losing trump is the only way to win. It compels *Z* to lead Diamonds while the command is with the enemy;

11. And thus, while *Z* cannot bring in his Diamonds, unless *B* and *A* make a double mistake (*B* discarding a Heart at trick 11, and *A* leading a Heart at trick 12), *Y* is forced either to discard a Heart, leaving *B* the full command, or his winning trump, leaving *A* to make his Club Eight.

12 and 13. *A* and *B* make three by tricks and the game.

NOTE.—The above game is from the *Westminster Papers*, and is interesting as one of the latest in which Mr. James Clay took part.

Our Chess Column.

BY MEPHISTO.

STEINITZ has challenged Zukertort to a match for a stake of about £200 a side, for eight or ten games up. The Chess world will be highly pleased and interested if a match will result from this prompt *défi*.

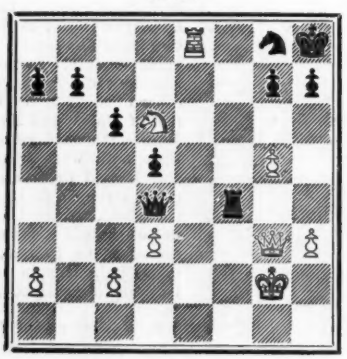
Blackburne, Mason, Bird, and Gunsberg will represent England at Nuremberg. This tournament promises to be a very great success, notwithstanding (oddly enough) that the prizes are very small.

The Counties Chess Association will, we believe, hold its annual gathering at Birmingham in the first week of August.

The following exceptionally fine ending occurred in actual play at Purssell's, Cornhill:—

AMATEUR.

BLACK.



WHITE.

J. ROBEY.

White to play and win.

PROBLEM No. 86.

BY C. PLANCK.

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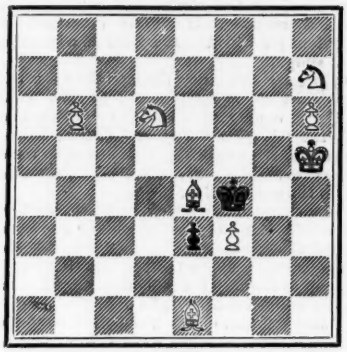

WHITE.

White to play and mate in three moves.

PROBLEM No. 87.

BY J. C. S.

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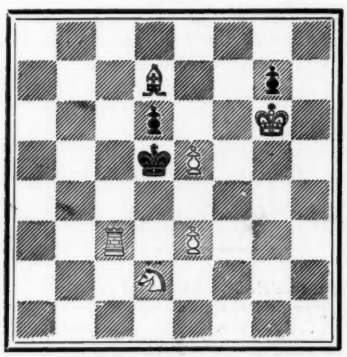

WHITE.

White to play and mate in three moves.

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess Editor.*

AMATEUR.—We do not know the book published in 1882.

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